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METHODS FOR THE PREVENTION OF SCARLET FEVER

AN ADDRESS DELIVERED AT THE
ANNUAL MEETING OF THE R. I. MEDICAL SOCIETY,
JUNE 9, 1888, BY
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It was only with the greatest reluctance that I yielded to the request of our retiring President and consented to prepare a paper for your entertainment to-day. In common with you all, I had been looking forward to the pleasure of listening to another delightful and instructive address from one whose words are always so fitly chosen and whose thoughts are those of a deep and thorough student of science, and I know that you would all be sharers in my disappointment that this could not be.

When the theme was suggested I could not but feel that the position of speaker and listener should be reversed, and that the health officer should learn from practicing physicians what methods of prevention they would desire his assistance in carrying out, and it was only in the hope that this discussion might initiate a freer expression of your views in regard to the nature and causation of the contagious diseases and a more perfect agreement in regard to the means which should be taken for their suppression, that I consented to appear before you.

Preventive medicine has made great strides in recent years, particularly in regard to infectious and contagious diseases. If the physician healed the sick merely as men lay bricks and cut stones, he would yet be bound by every dictate of humanity to remove the causes of disease whenever it was in his power. But as his profession is far above any mere trade, he should take the liveliest interest in whatever tends to prevent those ills which it is his business to relieve.



Then, too, to the physician as a man of science, the contagious diseases offer a most promising field of study, for they more than any others seem subject to general laws, and the recent discoveries in bacteriology and chemistry open up very inviting lines of research. Of these diseases scarlet fever is one of the most common and fatal, and was therefore selected for our special consideration.

In most systematic works on scarlet fever a merely perfunctory reference is made to its history and geography. But if we can get at the true facts of the distribution, spread and varying prevalence of a disease we often can, by a proper interpretation of them, arrive at a very fair conception of its nature. This is particularly true of scarlet fever. Where and when scarlet fever first appeared we do not know, but it has certainly prevailed continuously in Europe since the middle ages, and thence has spread to many other parts of the world. In 1735 it first appeared in this country at Kingston, Mass. It quickly broke out in Boston, a little later in New Hampshire, and gradually within a few years spread over New England, reached New York, and appeared in Philadelphia in 1746. Thence it extended down the coast and passed over the Alleghanies into Kentucky and Ohio in 1791 and 1793. In 1851 it appeared in California. It was carried to New Zealand and Australia in 1848. During the first part of this century it was imported into Madeira, where it disappeared in 1814, only to re-appear in 1824. In South America it is said to have been prevalent in 1796, but became extinct, and appeared again in Chili in 1829, and in 1831 in Buenos Ayres, whence it spread in 1832 to Brazil. It first appeared in Iceland in 1827, in the Bahamas in 1845, and it was carried to India in a transport ship in 1870.

We know that the aborigines of Africa, North and South America and Australasia were entirely exempt from this disease until the advent of Europeans. We know also that the early settlements were exempt, often for many years, and we know that, in some cases at least, the direct transportation of the disease can be traced. These general facts, taken by themselves and without the corroboration of other testimony, show almost conclusively that scarlet fever must be due to a material poison introduced from without the body, which poison must be intimately associated with the bodies of the sick. The hypothesis that the disease can be due to any atmospheric or telluric conditions is absolutely

untenable. We should cease to talk about mysterious epidemic influences. Specific, by which is meant infectious, diseases can only be caused by specific poisons, and though obscure meteorological conditions may favor or hinder the development and spread of these poisons, they cannot produce them. If scarlet fever can be carried in ships half round the globe, or in emigrant trains hundreds of miles across uninhabited continents, and, set free at the journey's end, spread without hindrance, it must be caused by a specific poison.

Moreover, the clinical history of this disease corresponds completely with what we should expect in an infection. It has a sudden, well marked onset, with rapid development of fever and other constitutional symptoms, often to such an extent as to cause death in a few hours, and yet with it all are to be found no serious lesions to account for all the disturbance. Then comes the efflorescence on skin and mucous membrane, followed by defervescence and exfoliation of the epidermis. It runs a regular course, and is self-limited like so many other infectious diseases. Its sequelæ, the nephritis, and glandular and serous inflammations, also point to infection. In its clinical history it is certainly very like variola, measles and diphtheria, diseases which are positively known to be both infectious and contagious. It is probable that in its nature also it closely resembles these. For these and many other reasons that will appear, scarlet fever is well nigh universally admitted to be infectious. But there are some who do not think that the question, Is it contagious? has been satisfactorily answered. If we turn to the leading writers on the diseases of children, or general medicine, we find that almost without exception they tell us that scarlet fever is contagious. Or, if we inquire of our medical acquaintances with the largest practice and greatest experience, we will probably receive the same answer. Great value must be attached to these opinions, not because authorities should be listened to simply because they are authorities, but because this generally received view is avowedly based upon a greater or less number of cases which fit perfectly with the theory of contagion. The number of cases which are best explained by the theory of contagion, and in fact seem only to be explained in that way, is so great that it is scarcely necessary to mention any in this paper. But some of them are most striking. When we

find a case of scarlet fever brought on board a ship—perhaps remaining there only a short time—and an epidemic abruptly develop after a few days, we have good evidence for contagion. And such instances can be found in the history of our own and the English navy. How often do we see a single case appearing in an orphan asylum, children's home, or hospital, quickly followed by case after case till a large portion of the susceptible children are attacked. An instance in point was the Shelter for Colored Children in this city, where 26 out of 29 inmates had never had the disease. After one case appeared the others took the disease in rapid succession till the whole 26 were affected. If one will take the trouble to examine the reports of the State Board of Health of Michigan, which often contain much information in regard to contagious diseases, he will find numerous and unquestioned instances where towns have been free from the disease until it was imported from another place, either near at hand, or distant hundreds of miles. Visits of the sick and convalescent or their nurses or attendants, the carrying of a body (accompanied by relations), the sending of clothing and bedding of the sick, are avenues of transmission which are clearly shown to be the source of the disease in a particular neighborhood, and from which it often spreads by steps which can be readily traced. A case is mentioned by an English writer where a scarlet fever convalescent attended a ball, and within a few days a large number of participants in the festival, living in different parts of the city, were attacked. The complete proof of the contagiousness of a disease is to be found in inoculation experiments, and if current statements are to be believed, such experiments, made both with blood and epidermis, have generally been found successful. Such experiments are said to have been made upon the human subject by several investigators, but I have not had access to their original reports.

Although it has been made certain by analogy, observation and experiment that scarlet fever is contagious, a very important and very practical question remains to be answered: Is scarlet fever always directly contagious? Is it invariably transmitted from one person to another, as is the case in small-pox, measles or syphilis, or does it, though emanating from the sick, develop independently of the body, as does the virus of yellow fever, typhoid fever and cholera? If we are to attempt to check the spread of this disease it is

necessary for us to know whether we have done everything needful by preventing direct contagion, or whether new foci of the disease may become established externally to the body, and the disease thus become endemic in certain houses or districts. One reason for assuming that scarlet fever does not develop outside of the body is its analogy to other diseases like variola, vaccinia and measles, which we know do not thus develop. Scarlet fever is much more like these diseases than it is like cholera or yellow fever. Another reason is that scarlet fever does not as a rule prevail so extensively in hot weather as in cold. The most prolonged wintry weather, a steady temperature at or below zero, does not in the least check the progress of this disease; on the contrary, it seems to flourish more at such a time, and to become less prevalent as summer approaches. The reverse is true of such diseases as cholera, yellow fever or malaria. If scarlet fever is a parasitic disease these facts are certainly very much opposed to the view that the poison can have an independent growth, for freezing certainly puts a stop to all animal and vegetable activity, and even chemical action is usually promoted by warmth and checked by cold. Then, again, if we study the extension of the disease or its prevalence in different localities, we find nothing to indicate that it develops outside of the body. Its prevalence is not conditioned by any meteorological phenomena, by moisture, heat, by filth, by saturation of the soil, or by any of those things which determine the extent and severity of yellow fever or cholera. No external forces or conditions have ever been found which seemed to influence the spread of scarlet fever any more than small-pox. I have never heard of the drinking water being even the vehicle of the virus, much less its nidus, as is so often the case in typhoid. No one on anything like sufficient evidence has accused sewers, cess-pools or drains, or any kind of decomposing filth of being the source of a scarlet fever epidemic. Milk has been suspected of containing the poison, and it might possibly serve as a medium for its growth, but if this ever occurs it must be rarely. Scarlet fever certainly does not develop in water or in filth. If it develops in the ordinary air of a room or on the surface of the walls or furniture, we should expect to find it diffused pretty generally over the whole of a house, yet the facts show that this is not the case, but that it does not require very much care in most cases to keep the disease

within the confines of one tenement. While scarlet fever does not follow the history of the extension of the miasmatic and indirectly contagious diseases, it does follow that of the directly contagious diseases.

The difficulty of tracing direct contagion is very great. Even in such a disease as syphilis, in which the mode of contagion is so well defined, it is not easy in every case to determine the true source of the poison. Pediculosis, the virus of which is certainly particulate, incapable of an independent existence, by no means microscopic, and for the transmissions of which either direct contact or the media of fomites is required, often eludes our most searching inquiries. Yet we do not on that account ever assume that the pediculus either lives off of the body or arises *de novo*. How much more difficult is it to determine the true source of a contagion when that contagion has not been isolated, and may retain its virility for a considerable length of time. It is true that we are to-day usually able to trace our cases of small-pox, though by no means always. But this is because each case is generally recognized and made public, and the aggregate number of cases is small. It was not easy to trace the contagion in each case in Montreal two years ago, or in any place before the era of vaccination. There must be nearly 150,000 cases of scarlet fever in the United States each year. That means 150,000 foci for the spread of the disease. In cities with the multitudinous means of intercourse, schools, churches, Sunday schools, dime museums, horse cars, hacks, circulating libraries, and the agency of laundresses, seamstresses, tailors, etc., and with the unrecognized and concealed cases which always occur, it is impossible to be sure of the source of contagion in every instance. In the country, however, where many of these conditions do not prevail, it is much oftener possible to do this, and the history of country epidemics shows that it is not localities, but persons which cause the disease. Even in Providence we are usually able to trace the source of contagion in about half the cases. An interesting series of such case was collected by Dr. Leonard. Starting from a single case in a rag-picker's family, the disease gradually spread in a way that could be readily followed through 12 families, causing 33 cases. Similar, though rarely as well marked series, have been several times worked out by Dr. Swarts. Such series it is true often occur in one neighborhood, and

might be attributed to local causes if it were not that those children who are not associated with the sick are the ones who escape. There were three school-houses in Providence last season which might have been thought at first sight, from the number of children attacked, to be themselves sources of infection. In one of these facts pointed so strongly to contagion in the school-house that it was closed for ten days and fumigated, and then occupied without a recurrence of more than a case or two, which were due to outside infection. After investigation showed that there was no doubt that during the spread of the disease unrecognized cases were attending the school. At the Borden Street School a large number of children were attacked, but they nearly all lived in one of the most densely peopled districts in the city, where the children are constantly playing with each other in the street and running into each other's houses. The children of the school who lived out of that immediate neighborhood generally escaped. At the Manning Street school there were a large number of cases, many of which were due to contagion spread (through ignorance on the part of the keeper), from a small candy store which the children patronized. Afterwards it was found that the family who kept the store derived the disease from some neighbors, by whom its nature was not at that time recognized. Among the wealthy people, making up a considerable part of the population of that immediate district, and who send their children to private schools, only two cases occurred. In almost every instance what appear at first sight to be epidemics of scarlet fever, prove on investigation to be epidemics spread by direct contagion.

If the poison of scarlet fever does not develop outside of the body, and we have no reason to suppose that it does, this disease must be propagated solely by contagion, or else it may at times arise *de novo*. The only way in which it can be directly and absolutely proved that it does not so arise is by tracing the contagion in each individual case of the disease, which we know would be impossible as long as the disease prevails as extensively as it does at the present time. On the other hand, only the most positive evidence should serve to convince us that the disease ever does occur spontaneously. We are certain that the other contagious diseases, at least some of them, do not arise *de novo*, and analogy alone should make us very slow in accepting any

other view in regard to scarlet fever. The contagious diseases must certainly be due to some specific material poison. This poison may be a living organism, as has been demonstrated in some of them, or it may be an inanimate chemical substance. If the former is true we cannot admit that these diseases ever occur except by direct propagation of the organism from generation to generation. In the animal and vegetable worlds we are not acquainted with such phenomena as the rapid development of new species or the development of identical species in many places and at frequent intervals of time. So far as the lifetime of man is concerned, and even during the entire period covered by history, species have remained reasonably fixed, and though rapidly propagating organisms like bacteria or infusoria may develop more rapidly than others, the careful study of these organisms has not shown any such changes. Contrary views have been advanced, but they are not supported by the best observers of the minuter forms of life. If we believe that contagious diseases are due to unorganized substances we can hardly imagine these substances produced except through the agency of living beings, otherwise we ought to be able to trace epidemic diseases to atmospheric or telluric influences, which we certainly cannot. If the hypothetical chemical substances which produce contagious diseases are the product of vital action, we have already answered the question of their *de novo* origin.

If it is possible we should like to isolate the specific poison of scarlet fever, in order that we may find its true nature and accurately define its mode of action; and ever since the so-called germ hypothesis made its appearance, attempts have been made to find the specific organism of this disease. Ecklund, of Stockholm, discovered some years since, in the blood and excretions of scarlet fever patients certain cellular bodies, to which he gave the name *plax scindens*. By him and others these organisms were certainly found in great abundance in all cases of scarlet fever, but his observations have not been generally verified, and the *plax scindens*, like so many similar organisms, is known no more.

Very many observers have found microorganisms, micrococci and bacilli in the blood, in the epidermal cells, in the epithelium of the mouth, throat and nose, and in other organs of scarlet fever patients. And it is to be noted that they have

not been found uniformly of the same kind and in the same locations, and also that many of the cases were evidently complicated with purulent or septic poisoning. Indeed, some of the organisms have been isolated and shown to be merely septic or pyogenic. Within the last few years two investigators have claimed to have isolated a specific organism of scarlet fever, and their views have been so extensively published, and have obtained such credence, that they are worthy of consideration. In 1885 an epidemic of scarlet fever in London was traced to a milk supply coming from a farm in Hendon. It was found that a number of cows in the dairy from which the milk came were suffering from a disease of the udders. This disease Dr. Klein proceeded to investigate. He found the disease characterized by the appearance of vesicles on the teats and udders, which in two or three days ruptured and formed ulcers which afterwards scabbed over. The animals were thin, and the hair came off in patches on the tail and back, but at the same time there were no apparent constitutional symptoms. The autopsy of two animals showed nothing beside spots of congestion or extravasation in the lungs, spleen, liver, kidney, etc. The disease was shown to be contagious by inoculation on other animals. From the original ulcers a micrococcus was obtained in pure culture by Klein, and inoculated upon two calves. One died in 27 days and the other was killed, and both showed congestion of internal organs, with numerous petechiæ; brownish spots were also seen on the palate, gums and lips. Afterwards Klein claimed to have shown that the micrococcus which he had obtained from the cows is found in the blood and tissues of scarlet fever patients, and produces a disease similar to that seen in the cows, and that he produced this disease in both calves and mice by feeding and by inoculation. He also found the same micrococcus in the blood of a monkey, which he believed died of scarlet fever. It is difficult to see how such views as these could have been accepted on such slender testimony. The disease of the Hendon cows with the ulcerated udders certainly seems far removed from scarlet fever, and the disease in the calves inoculated with the pure cultures of the micrococcus, does not resemble it any more closely. Even from Klein's own description we might fairly assume that he had simply separated from a local ulcer on a cow a septic organism with which he successfully inoc-

ulated calves and other animals. There is no reason to believe that any of the animals observed had scarlet fever or anything resembling it. Further light was thrown on this subject by the investigations of the Agricultural Department, which found that a large number of animals in herds from which the Hendon dairy was recruited, were suffering from what appeared to be vaccinia, and it is much more probable that this was the disease of the Hendon cows than that the latter was scarlet fever. But Klein did not isolate the organism of vaccinia even, but simply an exceedingly common and widely distributed pyogenic micrococcus.

About a year ago, Edington, of Edinburgh, published an account of his discovery of the specific organisms of scarlet fever. As was to be expected, it was entirely different from Klein's, being a bacillus instead of a micrococcus. Edington found the bacillus constantly present in a large number of cases of scarlet fever examined by him, but strangely enough, it was present in the blood during the first three days only, and then disappeared entirely, to return after an absence of three weeks, appearing then in the exfoliating epidermal scales. This bacillus Edington isolated by culture experiments, and was able to distinguish it from other similar organisms by its growth. This surprising absence of the bacillus during a prolonged interval would lead us to doubt its specific nature unless we could establish it by inoculation. And in fact we ought in any case to rely mainly upon this. Dr. Swarts, of this city, during the past winter, made a series of culture experiments both with the blood and epidermis of scarlet fever patients, but he did not, except in a few cases, find an organism resembling Edington's. He did, however, almost always find micrococci present, often of various kinds. Dr. Edington's inoculation experiments are not much more satisfactory than Klein's. He inoculated rabbits, guinea-pigs and calves with the bacillus. In these animals there was a more or less marked redness of the skin, sometimes diffused, sometimes in patches, desquamation of the cuticle, and shedding of the hair, with an accompaniment of considerable fever. In the calves there was marked redness of throat and enlargement of the papillae of the tongue. The autopsy of the calf that died showed lesions which might accompany any serious form of blood poisoning. This array of symptoms is hardly sufficient to demonstrate that the disease produced by the bacillus is really

scarlet fever. Moreover, the committee appointed by the Medico-Chirurgical Society to investigate this work of Edington's, have reported that they do not find the bacillus in all cases of scarlet fever, and that it does not invariably produce in animals the symptoms ascribed to it. They also showed that inoculations of scarlatinal blood and epidermal scales in calves produced only negative results. In fact it has never been demonstrated that the animals experimented upon by Klein and Edington are susceptible to scarlet fever. Edington has since inoculated a human subject with his bacillus without result. We must then conclude that the virus of scarlet fever has never yet been isolated. This is dwelt upon at some length, for this age seems to be a gala time for micro organisms, and we cannot be too careful in accepting what are daily offered as facts without sufficient demonstration.

Though no specific organism has been found in scarlet fever, we cannot help having a very strong belief that one exists. In fact we are not justified in thinking otherwise. As it has been incontestibly demonstrated that quite a number of infectious diseases are of a parasitic nature, and as this great class of diseases present so many points in common, and their history can be so well explained by the germ theory, so-called, and not so well explained in any other way, we ought in the absence of certain knowledge, to govern ourselves by the probabilities, and place scarlet fever among the hypothetical germ diseases. It is certainly to be hoped that the important work now being done upon the *role* of organisms and the complex chemical compounds formed by them in the production of disease, will lead shortly to the isolation of the virus of scarlet fever and the other exanthemata, whether it be an organism or not. Until that time we shall never obtain an exact knowledge of its action and modes of propagation. Nevertheless, much valuable knowledge has been acquired in regard to it, and it is for us briefly to review it at this time.

In regard to the period of incubation of scarlet fever, authorities differ, and fortunately it is of scientific interest rather than practical importance. The few inoculation experiments make the time from four to seven days. Very many claim that it may be extended to several weeks, but the latest and best observations, based chiefly on cases in which there was only one brief exposure, make it from

twenty-four hours to three days. Such cases are reported in the *Lancet* for 1883, in the current volume of *Science*, and by Murchison. The experience of the writer is also in accord with the belief that the incubation is usually within three days. In cases of prolonged incubation it is probable that the patient at the time of exposure was not susceptible to the disease, perhaps had no lesion of the mucous surfaces, but retained the poison about the person until susceptibility was acquired. In surgical and puerperal scarlet fever, which are merely accidental inoculation experiments, the duration of incubation is usually only a day or two.

The time during which the poison of scarlet fever is given off from the body, and the parts of the organism in which it is developed, are subjects so closely connected that they can be best considered together. There is very little evidence to show that scarlet fever can be transmitted during the period of incubation, though cases have been brought forward as evidence of it, but they are few in number and are entirely offset by the very much larger number opposed to it. In fact, it is doubtful if any of the contagious diseases are contagious until they are developed enough to give rise to recognizable symptoms. As to the preëruptive stage of scarlet fever there seems to be some good evidence to show that it is then contagious. Measles are certainly contagious during this stage and probably some other diseases. In scarlet fever the mucous surfaces become inflamed with the first rise in temperature and development of constitutional symptoms, and a certain number of cases are recorded where the disease seemed to have been acquired from a patient in this condition. As soon as the eruption appears, and the inflamed condition of the throat becomes well established, there is no question that the disease is decidedly contagious and that the contagion is given off from the body often for several weeks. Everything goes to show that the poison of scarlet fever is developed in the blood and also that it is contained in those organs where the poisoned blood seems to set up inflammatory processes, viz.: the mucous membrane and the skin, and perhaps the allied organs, the kidneys. If this is true we must expect to find poison given off first from the throat and probably at the same time in the discharges from the bowels, and also in vomited matters. Later it would come from the skin, particularly in the exfoliated epithelium, and perhaps later from

the kidneys in the urine. When we test these theoretical views by experience we find that patients certainly do transmit the disease from the development of initial symptoms until after the cessation of desquamation. The poison must then be given off from the throat and the general surface of the body. I have never found any actual proof that it is contained in the disjections or the urine, but it probably is. That it is contained in the epidermis we have positive evidence in the inoculation experiments of Stoll. Moreover, the great weight of clinical testimony is that the disease is much more liable to be transmitted during the period of desquamation than at any other time. We can be sure in assuming that the virus of scarlet fever is contained in the blood and is given off from the skin and the throat, the first points having been demonstrated by inoculation experiments, and the latter by abundant clinical evidence of the disease having been acquired by exposure to patients in the pre-eruptive stage. Indeed, some go so far as to say that it is more contagious at that time than any other. But the frequent removal of the well after exposure to this stage, and their subsequent immunity, show that this can hardly be true. Of thirty-one susceptible persons so removed in this city none were afterwards taken sick — at least until after a subsequent exposure, such as return to the house. Indeed we should hardly expect it to be otherwise, or our laboratory experiences show that pathogenic and other organisms are not readily, if ever, given off from moist surfaces. Hence the breath does not contain them except as it may carry particles of solid or liquid along with it. The very much greater contagiousness of measles during the pre-eruptive stage may be due to the abundance with which the virus is disengaged from the body by the profuse catarrhal discharges and the sneezing, etc., which occur at that stage.

How long the poison continues to be given off in scarlet fever is perhaps the most important part of the question. The English observers have better opportunities than others for determining this, as in most of their large towns isolation hospitals receive a very large proportion of their cases. By following up their patients when discharged and noting whether scarlet fever again develops in the family to which they return, valuable data can be obtained. It is the prevailing opinion that as long as the inflammation of the throat or tonsils continues, even in a slight degree, there is dan-

ger of contagion, but that in the case of the skin it is not necessary that the desquamation should have entirely ceased. That sometimes when the peeling has continued a long time the exfoliation contains no virus, it having been entirely excreted with the first layers. Others, however, hold that contagion is given off until the complete cessation of desquamation, no matter how much time may have elapsed. The actual practice in the English hospitals is to retain the patients for six or eight weeks. It must be remembered that in cases in which a prolonged communicability is reported it is very likely that fomites rather than the patients, was the source of contagion. Unless the clothing, hair, and finger nails of the patient were thoroughly disinfected, the poison might have been carried, stored, as it were, and freshly given off. In the forty-two cases in Providence in which removals were made, the time of absence varied four to eight weeks, in only four cases was it less than six weeks from the beginning of the attack, yet of these susceptible children only four were attacked on their return. These were away less than five weeks. The health officer of Salford, England, reports that out of 1,300 or 1,400 cases sent out from the fever hospital only three per cent. carried the disease to their homes, and in many of them the infection may have been only coincident.

Having determined with a fair degree of certainty that the poison of scarlet fever is given off from the first symptoms until the cessation of desquamation, we are led to the practical consideration of the chances of the carrying of this poison in fomites or by a third person. It is hardly possible to conceive of a contagious disease caused by a material virus, in which that virus cannot be carried. We know by clinical experience that it can be so carried in the case of measles and small-pox, yellow fever and cholera, and even in the venereal disease, and we should not be justified in believing anything different of scarlet fever without the most positive testimony. But the testimony of facts is all the other way. Medical literature is replete with instances which clearly demonstrate not only the contagious nature of scarlet fever, but also that its poison may become attached to almost any article, and thus be carried to a great distance. Many such cases were reported to the editor of *Science* in reply to a series of questions sent out by him in regard to the cause and prevention of scarlet fever. One

particularly striking case was that of a child who came down with the disease seven days after having in its mouth a letter which had been received from a person just convalescing. This case was reported from Michigan, and others of a similar kind were mentioned. In another instance a woman brought it from Canada in her clothing, and the case which contracted it from her was the means of spreading it through the district school. In another case a dress worn by a sick child was sent to a distant township. Again, a man living in a family in Detroit where there was scarlet fever, visited a young relative in a distant town, who was taken sick within a few days. A lady in Wyandotte, Kan., visited a family in another state where there was scarlet fever; a few days after her return one of her children sickened with scarlet fever, though there was not a case of the disease in town at the time, nor had there been for many months. A man in Belleville, Ill., had a child sick with scarlet fever; he drove fourteen miles in an open buggy to a farm where he took dinner and shook hands twice with a young lady who came down with the disease in a few days. A lady took care of scarlet fever cases and afterwards wore the wrapper she had on at that time while playing with some children, one of whom in a short time was taken sick with the disease and died. Cases were also reported where scarlet fever had been carried in clothing of various kinds, a picture book, toys, a pet cat, a wash-tub, etc., etc. In all these cases the evidence seemed very conclusive, as the children all sickened within a few days, and the neighborhood had for some time, often several years, been free from the disease. Many other cases of a similar nature are reported in the English journals, from time to time, and many are referred to in the article on scarlatina in Ziemssen. Such cases are so numerous, and the evidence offered is so conclusive, that there is no room for doubt that scarlet fever is frequently carried in all sorts of articles and also in the hair and clothing of attendants. It is often asked by the disbelievers in the contagiousness of scarlet fever, why it is not carried by physicians. It is carried by physicians, not only to their own families but to their patients as well. I have seen cases which I believed were caused in this way, but as I have before said, the evidence afforded by city cases is not always to be relied upon. Quite a number of cases are reported in *Science* where nurses and physicians have carried

contagion, sometimes to their own homes, and sometimes to others. And in several instances where a third person has carried scarlet fever, the exposure lasted no longer than a physician's visit. An instructive case was recently reported of an epidemic in a village in northern Europe in the winter time, where the children were all snowed in, and the doctor was the chief, and in some cases the only means of communication.

The question of the spread of contagion by means of milk is one which has received a good deal of attention of late years, and there is excellent proof that the disease is not infrequently disseminated in this way. Many cases are reported in English journals, but only one case was reported to *Science* by observers in this country.

Having determined that scarlet fever is contagious, and that it is often spread by fomites, it is important to know how contagious it is. That is, if a certain number of persons are exposed to it, how many will contract it, and what is the real danger of its being carried by a third person, or in clothing, books, toys, etc. What is the danger of its diffusing itself through the air of a house, school or hospital. It is only by knowing these things that we can hope to successfully combat the disease. Syphilis and small-pox are both contagious but they are contagious in different ways, and in one sense we might say in different degrees.

As to the degree of susceptibility we know that it varies very much in different persons and also in the same person at different times. We have all seen scarlet fever appearing several times in the same family, the second and third attacks affecting those who successfully withstood the action of the virus at previous exposures. There is no contagious disease in which every unprotected person succumbs at the first exposure. But the irregularity is very much greater in scarlet fever than it is in measles, whooping-cough or small-pox. Nearly every child exposed to measles for the first time takes the disease, but this is far from true in the case of scarlet fever, and this is one of the reasons which has led to the questioning of its contagious character. We know that children are more susceptible than adults. The fact that the number of cases reported during early childhood is very many times larger than in later life does not of course prove this, but the first historians of the disease in this, as in other countries, tell us that even when the whole

population is unprotected by previous attacks, it rarely affects adults. Children under one year are not very susceptible, as we frequently see the disease run through a whole family except the infant of a few months. The age of greatest susceptibility seems to be between the second and seventh years.

The following table shows the proportion of cases of scarlet fever occurring at different ages in this city during 1887 :

Age.	Cases.	Number per thousand of susceptible persons exposed.	Number per thousand of Population.
Under one year.	21	220	8.96
1	27	360	19.4
2	86	550	33.2
3	86	540	37.9
4	83	600	36.5
5	71	420	31.4
6	83	580	36.6
7	80	610	34.8
8	61	480	28.1
9	55	470	25.7
10	35	450	15.2
11	37	450	17.7
12	24	320	10.4
13	20	300	10.3
14	15	280	7.2
15	12	230	5.8
16	9	280	4.0
17	5	200	2.2
18	1	100	.4
19	3	230	13.5
20	4	400	17.2
over 20	36	690	

The number of cases over fifteen years of age and the unreliability of the statements of adults as regards their susceptibility render the percentages above fifteen of no statistical value.

During the present epidemic in this city, of 1,407 susceptible children exposed to the disease in their own families, 748 were attacked, which I think very nearly represents the average susceptibility, for in only a few cases was any attempt made at restriction. This simply means that if a child is exposed to scarlet fever, it has as good a chance of escaping as contracting the disease. But undoubtedly many, if not the most of these children, if again exposed even after an interval of only a few months, would be likely to be affected. Such, then, is the chance of infection by intimate connection with cases of the disease.

I have also collected some interesting facts which show that the danger of the spread of the disease outside the family, but in the same house, has been by many greatly magnified. Thus in 227 houses where there was scarlet fever and where there were other families with susceptible children in the same house, the disease was confined to the original family in 186 cases. When it is remembered that in most cases very little attempt was made to so restrict the disease, except to keep the well children out of the infected tenement, and that in many cases even this was neglected, we can see that the danger of the spread of scarlet fever from tenement to tenement in the same house is really quite small. It was also found that when fumigation was done the disease spread in only half as many instances as when it was not done, although the health department was provided with only very imperfect means for doing this work. We know also that in hospitals and asylums that rather imperfect means of isolation often prevent a general outbreak. On the other hand, when no attempt at restriction is made, as a general thing the disease prevails quite extensively.

All this should encourage us, as much as the facts before given in regard to the spread of the disease tend to discourage us. We must admit that scarlet fever, to which one-half of the unprotected youthful population are always susceptible, we must admit that this disease can be carried in books, toys, clothing, milk, etc., that it can be spread through the agency of nurses, physicians, and friends, even when they do not come in close contact with the sick, and

that it may be occasionally wafted by currents of air through the rooms of a house or the wards of a hospital, and that even a few minutes' exposure sometimes infects a person or fomites, yet we also know that notwithstanding all these avenues for the diffusion of the poison, a child may live down stairs, with scarlet fever up stairs, and stand only one chance in a dozen of catching it, provided he does not go up stairs. If the chance of diffusion is naturally so small as compared with either measles or whooping-cough, how much less could it be made by proper methods of isolation and fumigation.

It has been shown by laboratory experiments that in the infectious diseases the virus of which has been isolated, the chance of infection is greater the larger the dose. And it has also been shown that the chance of infection is small if not entirely *nill*, when the mucous and cutaneous surfaces are absolutely intact. These facts help to explain many of the apparent vagaries in the spread of scarlet fever as of other contagious diseases.

As a summary of our knowledge of scarlet fever it may be said that it is a contagious disease, the virus of which behaves exactly as if it were a living organism, that it probably does not develop outside the living body, it is probably received through either the alimentary or respiratory mucous membrane, after a brief incubation the disease is established and the poison is thrown off from the mucous and cutaneous surfaces as long as inflammation exists or desquamation continues, and is thus disseminated in the air and attached to various articles, is carried from place to place, retaining its vitality for many months.

Accepting these facts we must devise a plan based upon them to prevent the spread of this disease. We must have in the first place isolation of the sick. How can this be obtained? In the houses of the well to do and rich I believe that this can be perfectly obtained. But it cannot be if those who care for or come in contact with the sick are allowed to mingle freely with other people in their usual avocations. As soon as the disease is recognized, which is usually with the eruption, often before, the patient should be placed in a room or suite of rooms, as the case may be, and kept there until desquamation has ceased. All unnecessary articles, particularly drapery, should be removed from the room before the patient is put in it. The reason

for this is simply the difficulty of disinfection. The room can be isolated by means of a wet sheet hung before the door, if it is wet with corrosive sublimate so much the better. To secure perfect isolation the attendant should remain constantly in the room and not run all about the house and have intercourse with all the inmates. As this can rarely be possible, she should wear a cap and a starched cotton dress, never a woolen one, and keep as much away from the rest of the house as *possible*. All dishes used in the room and all clothing can readily be put in a disinfecting solution, before they are carried out. But I suppose that plain water would be just as efficacious, provided they are afterwards boiled as they should be in all cases. Of course all discharges from the patient should be disinfected or destroyed before leaving the room. If it is necessary for any one besides the immediate attendant to enter the sick-room he should not mingle with other people until he has changed his clothes, and the head should be covered while in the room. This rule applies to doctors as well as to family and friends. Though there is but little chance of a physician carrying the disease to his patients, such cases have occurred. Moreover, it is difficult to secure rigid isolation on the part of the family, as long as they can refer to the doctor's constant passage from the sick to the well. Of course all this is a trouble, a very great trouble, but so it is a trouble to have thousands of children die every year of this malady. And certainly we have every reason to believe that if we take the trouble we can greatly restrict the disease. Provided such isolation is honestly carried out, I can see no danger of the rest of the family contracting the disease, or any objection to their mingling freely with other people.

I am inclined to think that we have a means of making much more effective the above method of isolation, and perhaps of rendering it almost unnecessary. Inunctions have long been employed in a haphazard sort of way for their benefit to the patient. Some have, however, strongly urged their use as a means for preventing the spread of the disease. Among these is Jamieson, of Edinburgh, who has never failed to prevent the spread of the disease in families by the use of inunctions. At the same time he applies to the throat several times each day a solution of boroglyceride. But I do not see how the latter can be more than partially effect-

ive. The inunctions, as he uses them, are made morning and night, and include every inch of the superficial surface of the body, including the head. They are begun as soon as the disease is recognized. Jamieson uses carbolic acid, 15-30 grains; thymol, 10 grains, to the ounce of ointment. A warm bath is given the patient every night from the first. Jamieson claims that this treatment effectually destroys the virus in the skin, and that if there is no throat affection the patient may, as soon as his own health permits, freely mingle with others without danger of spreading the disease. Jamieson has for four years treated patients in this way without ever having the disease extend beyond the initial case, though no isolation was attempted. Edington's experiments, though not dealing with the specific organisms of the disease, indicate that the skin is pretty thoroughly disinfected by the process. And even if the disinfection is only partial, the inunctions must almost, if not entirely, prevent the diffusion of the poison-bearing epidermal scales. This method should certainly have a fair trial. And this it certainly never has had, for in 476 families in this city inunction was practiced in only 186 and in almost every instance it was very imperfectly done.

In most cases occurring in the families of mechanics and laborers, it is entirely impracticable to isolate the patient, and impossible for the rest of the family not to come in contact with the world in their daily work. There is only one thing we can do in such cases, and that is remove the patient to the hospital. Unfortunately we do not have many such hospitals in this country; we have only just gotten one in this city and that by gift, not from the public funds, and when we do have them they are not much used. In England, the majority of their large towns and many small ones are well provided, and the prejudices of the people, though decided at first, have been so far overcome that a large proportion of cases are there treated. In Bradford, a manufacturing town larger than Providence, 75% of all cases are treated in the hospital. It is in the closely crowded tenement house districts that scarlet fever is most rampant, and we must expect to meet with great discouragement until such accommodations are provided. In Leicester, England, where small pox is successfully restricted, in an unvaccinated population, patients with that disease are accompanied to the hospital by mother or friend, and a sim-

ilar practice might well be followed in other diseases. Until isolation can really be secured, either at home or in the hospital, or until the restrictive power of disinfectant measures shall be determined, we cannot stamp out the disease, though we may reasonably hope to restrict it. We should, however, do all we possibly can in this line, and we shall be repaid if by it only one child be saved.

The health officer can control pretty well the school attendance and the spread of the disease through school and library books. By the co-operation of the clergy, which he can readily obtain, he can also control Sunday-school attendance and the infection of Sunday-school books. He can forbid public funerals at infected houses, and prevent the use of public conveyances on such occasions, and by the sick. The city and towns should also provide for the proper disinfection of infected rooms, clothing, etc., at the public expense, and cities should have an ambulance for the removal of cases of contagious disease, when it becomes necessary. The health officer can also put a placard upon the house for the purpose of warning any person who might unwittingly expose himself to contagion, or carry it to his children, and also for the purpose of teaching the public that scarlet fever is a disease to be avoided, a fact of which many appear to be woefully ignorant. But the playing of infected children with the well, their riding in horse-cars or other public conveyances, attendance at public resorts, the taking in of washing and dress-making at infected houses, the health officer finds it difficult to prevent, except so far as he may disseminate knowledge on these points. In all such cases the physician can do much more than the official in inculcating a proper observance of sanitary rules. Unfortunately, however, those people who most need such advice either do not have a doctor at all in mild cases, or else allow him to make but one or two visits. In such cases, at the request of the medical attendant, the health officer should make frequent visits for the purpose of directing, supervising, and if necessary, giving assistance.

In order to do all this, the health officer must have a knowledge of each case, which can be best obtained from the attending physician. Such information the physician should be ready to give, both for the sake of advancing our knowledge and for the benefit of humanity. But the State certainly ought not to ask for such reports without providing a payment for them.

How long isolation should be maintained is, as we have seen, a mooted question. In the majority of cases it is for five or six weeks, or until desquamation shall have ceased. In the absence of constant supervision, an arbitrary time has to be fixed by the health department. In this city it is five weeks, which I believe is ample for most cases, but not long enough in a very few. After the period of isolation is ended, the patient should be bathed thoroughly, including the scalp, each day, for several days. In fact, this ought to be done daily during convalescence. Then comes disinfection. The best disinfection for linen is boiling; for wood-work or anything that can be washed, corrosive sublimate, one part in two thousand, for the room, bare of everything, burning sulphur. For fabrics that cannot be washed, the only disinfectant is heat, and steam heat is to be preferred. It should be supplied free of cost in all our cities and large towns. Whatever expense is entailed by the patient's family in preventing the spread of contagious disease should, it seems to me, be borne by the State.

Such methods for the management of scarlet fever should, theoretically, diminish the number of cases. We naturally then turn to statistics to see if our theories can be verified. We find that during the past fifteen or twenty years there has been a general diminution of deaths from scarlet fever, but that this diminution has shown itself even more markedly in other zymotic diseases. The decrease in the number of cases of these diseases is doubtless due to the better care which people take to-day to avoid contagion and infection than they did twenty years ago. Both English and American statistics well illustrate this point. In Massachusetts, the death rate from scarlet fever was only half as great during the past fifteen years as it was in the preceding fifteen years. In Rhode Island, the decrease is not so great. During the fifteen years preceding 1885, scarlet fever caused 3.65 % of all deaths, and for the fifteen years preceding 4.63 %. In Providence, during the first half of the last thirty-two years, scarlet fever caused 5.38 % of all deaths, and during the last half 4.25 %. It is also shown that in scarlet fever, as in the other zymotic diseases, the decrease is greatest among the more intelligent people, for we know that it is among them that the greatest care will be taken to avoid contact with the sick. Statistics show that there has of late years been very much less scarlet fever

in the wealthy sections of this city than in other parts. All this, however, only indirectly shows the value of preventive measures. It is only within a few years that any serious attempts have been made to check the spread of scarlet fever, even in the public schools of Providence, and it was only last October that the placarding of houses, the forbidding of funerals, etc., etc., was begun. Previous to that time, in every epidemic which had occurred in Providence, the death rate increased up to January or February, as happens everywhere when nothing is done to prevent it, but the present epidemic began in September last year, and reached its height about the last of October, and then diminished, pretty steadily, until the present time. It would, of course, be an assumption to claim that the increased care which was taken to prevent the spread of the disease was the actual cause of the shortening of the epidemic, but it certainly tallies with the results obtained in other places. We have also seen that the particular efforts made, such as removals and fumigations, were followed by recognizable good results.

We have in the reports of the State Board of Health of Michigan conclusive evidence of the value of even imperfect methods of isolation and disinfection. During the year 1886, of 45 outbreaks of scarlet fever in that state, where isolation and disinfection both were neglected, the average number of cases was 13.84 and of deaths 1.02. In 59 outbreaks where isolation and disinfection were enforced, the number of cases was 3.03, and of deaths .22, a saving of over 75 per cent. in favor of methods of restriction. This is certainly very encouraging, particularly when we know that in most cases the isolation could not have been perfect, and that the disinfection could only have partially destroyed the virus. It almost makes us think that it would not be such a herculean task to completely stamp out the disease, at least in rural communities. In large towns and cities the work must be much more difficult, but even there we can and should obtain much greater success than we ever have hitherto. To accomplish this there must be a hearty co-operation on the part of the medical profession with the health officer, and a certain degree of unanimity in methods of management. That there will always be careless and willful opponents of the public good within the profession, and still more without, I fear is true, and for these clear and

stringent laws capable of speedy and impartial execution, should be provided. And I think we have every reason to hope that if the medical profession of this or any other community will recognize that scarlet fever is always a contagious disease, and will agree upon proper methods for its suppression, and will diligently instruct their patients in them, they can secure from the local or state government all the assistance they need, and can succeed in making scarlet fever as rare as typhus fever or cholera. The medical profession can find no more promising field in which to exercise its philanthropy than the contagious and infectious diseases, and as every year brings us new and more effectual methods of prevention we shall find even the busiest of physicians encouraged to renewed vigor in a warfare of extermination against these dread scourges of the human race.

